PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Prevalence of mental ill health, traumas and post-migration stress among refugees from Syria resettled in Sweden after 2011: a population-based survey
AUTHORS	Tinghög, Petter; Malm, Andreas; Arwidson, Charlotta; Sigvardsdotter, Erika; Lundin, Andreas; Saboonchi, Fredrik

VERSION 1 – REVIEW

REVIEWER	Cornelis J Laban, MD, PhD, psychiatrist Community Mental Health Institute, GGZDrenthe, De Evenaar, North Netherlands Centre for Transcultural Psychiatry, Belien/Leeuwarden The Netherlands
REVIEW RETURNED	18-Aug-2017

GENERAL COMMENTS	This is a very welcome and excellent study. So far it is the first larger study among Syrian war refugees in Europe on mental health items. The design is solid, the instruments are recognized tools to measure MH among refugees and the statistics are clear and straight forward . The number of participants is high(although the response rate is low) A mayor comment is : how does post migration stress influence / moderate the relationship between PTE's and the mental health outcome measures . The authors analyze them PTE and PMS separately but it would be very interesting to also include them in one analysis . I recommend to do this, My other comments are: Abstract The items design, setting , participants and main outcome measures can be placed under one heading : Method for clarity I suggest you add that you used a series of logistic regression analysis. It a strong characteristics of your study Intro No comments, clear. Methods Page 5 : translation : you mean Experts from within the Syrian community ? . Can this expert group be called a focusgroup ? And did you also involve this experts in formulating the 'most common types of refugee related PTE (later on page 5) ? – Now you only mention 'the scientific literature' Page 5 : the term 'cognitive interviews' is not widely known, please explain. Is TAP one of the ways you cn take a cognitive interview ? Page 5 : refugee related pte ; I do understand pre-migration, but what is peri- migration, ?

	I assume you mean the period between leaving Syria and arriving in Sweden, or … ? Please explain PTE : I do not know the phrase : at close quarters, I think I am not the only one, please explain/rephrase . PTE : what is the difference between physical violence or assault and torture . ? was this difference explained to the participants. This might be important in light with your finding that torture gave a 'puzzling' result in your analysis . (if you combine the two items, does it make any difference in the result ?) Page 6 : Mollica is using 2.5 as cut off score for a clinical relevant PTSD, you use 2.06 Post migration stress : why did the authors not include the stressor "lack of work or work below level' , lack of finance does not cover this item, because people might be satisfied with the allowance they get , but they want to get work, (has to do with self esteem eg) Statistics, no comments, clear, nicely explained. Results In table 1 Title : I suggest Sociodemographic char and non response analysis instead of Non response analysis and sd c . Table 1 : year of immigration : only 6.5 % arrived in or before 2011 , am I right ? if so the sign ≥ should be ≤ (2011) Page 9 , table 2 : did you do an analysis on the associations between year of immigration and MH . I suggest : If significant results appear, add them to the table, if not, mention the finding in the text. As you know some studies show a decline in prevalence rates, some not. (In the case of asylum seekers some studies show an increase) Page 9 Quote : The strongest correlations and comorbidities ae found between depression and axiety and between PTSD and depression…….. More precisely is : …. between anxiety and depression (86.6 (83.1-90.0)) , Page 11 : It says : Around 20 % of the respondents reported that they often had felt excluded etc. This is post migration stressor and should be mentioned in this subparagraph . Discussion The finding that education is not related to MH is supported by the review of Bogic, but porter and haslem found that a high education was a riskfactor for MH """"More-educated refugees scored lower on mental health indices than less educated refugees(Q=319.68;P .001; R2=0.28),as did those with higher predisplacement socioeconomic status (Q=177.71; P .001; R2=0.16)."""" In their discussion paragraph they write ; """"However, higher levels of education and socioeconomic status before displacement, considered by some to have buffering functions,19 were associated with worse mental health outcomes in the analysis. Greater predisplacement intellectual and economic resources may imply a greater subsequent loss of status rather than a protective effect on refugees against their predicament. " I think this is a very important item. In clinical practice you see highly educated person suffer from the lack of opportunities for work, let alone for work on their own level. Government and business people should create much more chances for integration on the labour market. The finding that 'felt sad because not reunited with family members' was no longer a significant predictor of any mental ill measure (page 12) contradicts with the clinical experience and the literature mentioned (12,13, 32) does not support this finding either.

	I think more discussion is needed, what might be the background of this finding, while lack of (family) social support is so often found as a riskfactor for MH (Limitation Ok Conclusion The government can also be addressed when it comes to give more attention to adequately support I think . The PTE are in the past, but the PMS in the here and now and interventions can decrease them . Also health workers can be addressed, because they should seriously pay attention to the PMS and not only focus on treatment of trauma's rom the past. A resilient oriented approach is recommendable. If the comparative risks of PMS is found be higher that those of PTE your 'case 'is even stronger.

REVIEWER	Raija-Leena Punamäki Faculty of Social Sciences/ Psychology FIM-33014 University of Tampere ADDRESS: Kalevankatu 5, Linna 4krs, Finland
REVIEW RETURNED	21-Aug-2017

GENERAL COMMENTS	The study examines, first, the prevalence and comorbidity of mental health problems (psychiatric disorders or symptoms of depression, anxiety, and posttraumatic stress, PTSD) in a representative sample of 1215 Syrian refugees settled in Sweden since 2011. Second, it analyses the role of traumatic war events and post-immigration stressors in associating with mental health problems (Mental ill health). The topic is very important and timely, as many refugee and asylum seekers attempt to heal their war-related psychic wounds alone, with the help of other people, and in the western health care systems. It is thus important to learn epidemiologically about the severity and contributing factors of mental health in the vulnerable group of refugees. The study has some important strengths, including substantial sample size (although 70%- drop-out), high internal consistence of main mental health variables, and professional statistics providing sensitivity analysis and large number of very detailed Tables. The manuscript serves criticism for the quality of scientific argumentation for the research questions (Introduction), including too old literature of trauma research and narrow reporting of earlier empirical findings among refugees in Europe. The manuscript would need more scientific conceptualization and structuring of Introduction (including current research questions) and the Method and Results sections. Hopefully the critical remarks below help the authors to rewrite their manuscript. A thorough rethinking and reworking would improve the manuscript. Introduction. The introduction does not follow the principles of scientific writing. The authors should present more sufficiently and informatively the available research on mental health (mental ill health) among refugees in western or Nordic countries. They have chosen one review (Bogig et al.) that is adequate, but they have not conducted a thorough literature review on war trauma, mental health and on underlying psychosocial factors and processes.

	Learning about earlier literature would be necessary in order to be able to contribute to the field of refugees' mental health. Currently the reporting is highly technical, e.g., reporting so large differences as 2% and 80% in disorder prevalence is not informative. The introduction should be written in a more problem-focused and conceptualized manner. The reader is curious and wants to know e.g., why one refugee group would show 4% of PTSD, while in some another groups 86% suffer. If it is important to give this information (which I doubt), the authors should report that maybe the refugees with 86% disorders were a patient group,.seeking help in health care, or arrived just from concentration camp or something that makes sense. The current technical writing without problem-focused orientation is not informative. In addition to Bogig, the authors should review earlier research e.g. by C. Panter- Brick, M. Fazel, T. Beatancourt, E. Hauff, or D. Silove. The reference of Mollica et al. and Carlsson et al. are from 1990's and concerning Cambodian refugees in USA. Thus more contemporary literature should serve as an argument how the current research questions are contributing something new to the literature. The research task needs more clear conceptualized of the setting. The authors list issues like mental health, traumatic events and stressors as they would be the same phenomenon. They should make clearer what are e.g. war-related traumatic events and potentially traumatizing events (PTE), and clarify the relations between war trauma, post-migration stressors and psychiatric disorders. Research aims. Usually the introduction ends up with clearly stated aims/tasks/research questions/hypotheses of the study. Currently there is not a separate paragraph for the aims. Due to the poor conceptualization of the research setting (war trauma and refugee-immigration -related stressors), the research aims are highly explorative. In the Result and Discussion part authors write that some findings were "unexpected" or "expected", which means that they had had implicit hypotheses. The earlier research on the topic of refugee mental health and related factors is extensive, which legitimates the formation of hypotheses. The current writing is rather careless, e.g., authors state "second aim", but miss "first aim". The second research question should be reformulated so that it would be understandable as such, alone (e.g., not "four studied mental ill health measures"). The research task of analysing the prevalence of PTE and post-migration stress (indicated by single items constructed to the present study) is not a scientifically important or new question, and would rather be placed in Descriptive statistics or Description of the sample. Methods. The section does not follow the "common" logic of research reports (also not according to the BMJ-open guidelines). It would be more reader-friendly to apply "traditional" reporting style. (Participants: Who participated, from where they were recruited, selection criteria etc., Study Procedure: How the study was conducted and how data were collected and analyzed. Measures: How the concepts were constructed to be assessed). Currently, the Method starts with Procedure (or even mailing of questionnaires), and the whole text (two pages) is difficult to follow, and the reader has difficulties to find the basics information. Drop-out or attrition analysis is missing. The Statistical analysis -section should be written so that it follows the research tasks or questions (after their reformulation). The paragraph "Participant involvement" could maybe moved together with information on piloting the research tools and setting.

	(Currently defined as “Usability of the questionnaire was tested by cognitive interviews conducted in a rehabilitation center for war and torture trauma patients, with ten patients with Arabic as their mother tongue”). Results. The results section is extensive with a large number of Tables (and Supplement tables). In this sense, the manuscript seems more a rapport than a scientific paper. The reformulation of research questions may help. Thus, prevalence of PTE and post-immigration stressors would not be research task but they belong to the descriptive statistics. It is difficult to understand why authors have chosen to analyze the association between trauma, stress and mental health by using single items of war-related traumatic events and post-migration stressors. More preferable would be to construct sum variables, and apply for instance step-wise regression models to learn what factors contribute to the refugees’ mental health. The single associations seem more as a primary analysis. Discussion. The idea of discussion is e.g., to (1) summarize own main findings in a more abstract and theoretical manner (not only repeating the results), and combine them to earlier studies, phenomena and theories to illuminate the significant of the findings. (2) Discuss each main finding or combination of findings in relation to earlier studies. Building theoretical arguments to deepen the understanding of the phenomena is important (e.g., here the factors that explain the variation of ill health after potentially traumatized refugees, why does vulnerability occur). In the current text, there is not really discussion of the results, but the text repeats the findings. The idea of discussion is to integrate information, to elevate concrete findings on a more abstract (theoretical) levels. The discussion should thus be rewritten in the normative scientific ways. The authors consider their research the first study “of this kind” and specify rightly “Syrian refugees settled in Sweden”. Yet, that is not enough; the authors may discuss how and why Syrian war trauma may differ, e.g., from other war-survivors settled in more peaceful countries. The findings concur with earlier studies (women vulnerable, disorder prevalence, exposure to trauma associates with high risk for disorders). Maybe a more thorough familiarizing with existing research would have brought new contributions to the field. Yet, the replications are also valuable, but the replication of earlier findings is here based here on single items of trauma and stress, which is open to criticism.

VERSION 1 – AUTHOR RESPONSE

Reviewer 1

General comment
This is a very welcome and excellent study. So far it is the first larger study among Syrian war refugees in Europe on mental health items. The design is solid, the instruments are recognized tools to measure MH among refugees and the statistics are clear and straight forward . The number of participants is high(although the response rate is low)

A mayor comment is : how does post migration stress influence / moderate the relationship between PTE's and the mental health outcome measures . The authors analyze them PTE and PMS separately
but it would be very interesting to also include them in one analysis . I recommend to do this,

Response: Thank you for your encouraging view of the study. We certainly agree that that it would be very interesting to examine whether, and to what extent, PMS factors moderate the associations between PTE's and the mental health outcomes. However, fitting such analyses within the present manuscript would require an extensive number of additional and in-depth analyses addressing each of the eight included PTEs, seven included PMS and the multiple outcomes. Given the very large number of required analyses on basis of the combinations of the multitude of the included variables necessary to address this question, we feel that this approach is somehow beyond the scope and the aim of the current study. We have therefore opted to primarily address the establishment of prevalence of mental ill health and its correlates among Syrian refugees here, and to study the interactions between the large number of potential antecedents of these outcomes in future studies with a more comprehensive treatment of the included variables.

Furthermore, the deliberate choice not to adjust for PMS factors in examining the association between PTEs and the mental health outcome measures was made in order to avoid over-adjustment in the analyses. Since PMS may be influenced by both outcomes and exposure, and as it is likely a mediating rather confounding factor, such over-adjustment would have resulted in biased estimates. We have revised the manuscript in order to explain this analytical strategy more clearly (see paragraph 4 page 8). However, to approximate this mediation pattern empirically, the revised version now includes a set of crude mediation analyses as additional sensitivity analyses, in which the number of types of PMS and PTEs are constructed as continuous variables. These analyses provide a crude approximation of the extent of the direct and mediated associations between PTEs and mental health with PMS as mediator (see paragraph 9 page 8, and paragraph 1 page 14).

My other comments are:
Abstract
The items design, setting, participants and main outcome measures can be placed under one heading : Method for clarity I suggest you add that you used a series of logistic regression analysis. It a strong characteristics of your study

Response: Thank you for this suggestion, we have now added "a series of logistic regression analyses" into the abstract. We prefer to keep the structure of the abstract as it is, if not the editor object.

Comment: Intro
No comments, clear.

Methods
Page 5 : translation : you mean Experts from within the Syrian community ? . Can this expert group be called a focusgroup ? And did you also involve this experts in formulating the 'most common types of refugee related PTE (later on page 5) ? – Now you only mention 'the scientific literature'

Response: yes, we mean "experts within the Syrian community" and we believe that this expert group can be labeled as a sort of "focus groups". The manuscript have been revised accordingly (see paragraph 2 page 6). These experts were involved in discussing appropriate translations and adapting formulations of refugee-related PTEs, but the items in themselves were retrieved from the scientific literature. We have now also added a reference of a recent publication (Sigvardsdotter et al) in which the developing of this checklist is thoroughly described.

Comment: Page 5 : the term 'cognitive interviews' is not widely known, please explain. Is TAP one of the waysyou cn take a cognitive interview ?

Response: We regard TAP as a structured form of cognitive interviewing. We have in this revised version omitted the term "cognitive interviewing" to avoid potential confusion.

Comment: Page 5 : refugee related pte ; I do understand pre-migration, but what is peri- migration, ? I assumeyou mean the period between leaving Syria and arriving in Sweden, or … ? Please explain
PTE : I do not know the phrase : at close quarters, I think I am not the only one, please explain/rephrase.

Response: To clarify what peri-migration mean we have added the following: "(peri-migration period does in this context refer to period between leaving the home in Syria and arriving to Sweden)" (Paragraph 5, page 6). To clarify what is meant with "war at close quarters" the following has been added "(i.e., close proximity to war combat)" (Paragraph 5, page 6). Furthermore, the TAP protocol (i.e., cognitive interviews) did not reveal that the respondents had difficulties to understand the Arabic translation of this item.

Comment: PTE : what is the difference between physical violence or assault and torture? was this difference
explained to the participants. This might be important in light with your finding that torture gave a 'puzzling' result in your analysis . (if you combine the two items, does it make any difference in the result ?)

Response: Physical violence may indeed be a part of torture. But all physical violence is not torture. For something to be regarded as torture, according to the definition we apply, it need to be an intentional act of violence (physical or mental) with the intent of compel action, break down individuals psychologically and or gain information. Violence does not have to be this deliberate and driven by such intents. There is of course a substantial overlap between the concepts, but they are nonetheless different concepts.

We chose not to provide the respondents with a precise definition of torture as previous studies have shown that individuals generally have a very similar conceptualization of what torture is and that these conceptualizations are much in line with the common definitions of torture (Westermeyer J., Hollifield M., Spring M., Johnson D., Jaranson J. Comparison of Two Methods of Inquiry for Torture with East African Refugees: Single Query Versus Checklist. Torture 2011, 21, 155-72. And Montgomery E., Foldspang A.

Criterion-Related Validity of Screening for Exposure to Torture. Danish Medical Bulletin 1994, 41, 588-91.). According to the TAP protocol there were no indication that respondents for example interpreted “torture” according to UN definition (i.e., that it needs to be state “sponsored”). When combining the “torture” and “physical violence” items the risk estimates are between those reported for “torture” and “physical violence” in the tables.

Comment: Page 6 : Mollica is using 2.5 as cut off score for a clinical relevant PTSD, you use 2.06
Post migration stress : why did the authors not include the stressor “lack of work or work below level’ , lack of finance does not cover this item, because people might be satisfied with the allowance they get , but they want to get work, (has to do with self esteem eg)

Response: the 2.5 cut-off score that often is used to determine clinical relevant PTSD is from an old study regarding Cambodian refugees. We instead used the 2.06 cut-off score on the basis of a more recent study among individuals from Kosovo (Mollica is in fact one of the co-authors of that paper as well). We believe that fairly recent Kosovo population is perhaps more comparable to the Syrian population than Cambodian population. Still, it is of course difficult to exactly know how suitable the 2.06 cut-off is for Syrian refugees. This issue have now been included as a limitation in the discussion section of the manuscript.

We also agree that additional PMS factors of importance may not have been included or not assessed sufficiently in this study. We have now more explicitly pointed out this as a limitation in the discussion section (see paragraph 5, page 17).

Comment: Statistics, no comments, clear, nicely explained.

Results
In table 1 Title : I suggest Sociodemographic char and non response analysis instead of Non response
analysis and sd c .

Response: Thank you for the suggestion. The manuscript have been revised accordingly.

Comment: Table 1 : year of immigration : only 6.5 % arrived in or before 2011 , am I right ? if so the sign ≥
should be ≤ (2011)

Response: Thank you for spotting this! The manuscript have been revised accordingly.

Comment: Page 9 , table 2 : did you do an analysis on the associations between year of immigration and MH . I suggest : If significant results appear, add them to the table, if not, mention the finding in the text.
As you know some studies show a decline in prevalence rates, some not. (In the case of asylum seekers some studies show an increase)

Response: Yes you are right. No significant associations (overlapping confidence intervals) were however detected. We have now added a sentence about that in the manuscript (see paragraph 1, page 11)

Comment: Page 9 Quote : The strongest correlations and comorbidities ae found between depression and axiety and between PTSD and depression…….. More precisely is : …. between anxiety and depression (86.6
(83.1-90.0)) ,

Response: This sentence refers to both the comorbidity analyses (the four top rows) and the correlation analyses (the four bottom rows). The correlation between depression and PTSD is Phi 0.67, while correlation between anxiety and depression is in fact slightly lower (0.66). Moreover we see in table 3 that 90.0% (86.8-93.1) of those with PTSD also have concurrent depression. Thereby we feel that original sentence represents the findings presented in table 3 well. But maybe we have misunderstood this comment. Please let us know if that is the case

Comment: Page 11 : It says : Around 20 % of the respondents reported that they often had felt excluded etc. This is post migration stressor and should be mentioned in this subparagraph .

Response: Yes it is indeed Post migration stress. Our intention was here to have two paragraph to present the major findings regarding post-migration stress shown in table 5. In the first paragraph were we write “Around 20 % of the respondents reported that they often had felt excluded” are used to say something about the prevalence of the different types of PMS that we investigate in this study. In the second paragraph we report results regarding the associations between the different types of PMS and the mental health outcomes. So in accordance to this way of structuring the result about PMS we feel that we have incorporated this particular sentence in the correct paragraph.

Comment: Discussion
The finding that education is not related to MH is supported by the review of Bogic, but porter and haslem found that a high education was a riskfactor for MH “”””More-educated refugees scored lower on mental health indices than less educated refugees (Q=319.68;P .001; R2=0.28),as did those with higher predisplacement socioeconomic status (Q=177.71; P .001; R2=0.16).””” In their discussion paragraph they write ; “””However, higher levels of education and socioeconomic status before displacement, considered by some to have buffering functions,19 were associated with worse mental health outcomes in the analysis. Greater predisplacement intellectual and economic resources may imply a greater subsequent loss of status rather than a protective effect on refugees against their predicament. “ I think this is a very important item. In clinical practice you see highly educated person suffer from the lack of opportunities for work, let alone for work on their own level. Government and business people should create much more chances for integration on the labour market.

Response: Thank you for this input! We do agree that a greater loss of status may be a likely explanation to why we were unable to substantiate any associations between education and mental ill health. We would argue that this may be due to that the positive (buffering effect) of higher educational level is canceled out by its adverse effect. i.e., greater loss of status. We have now in this revised version expanded this discussion in line with the reviewer’s suggestions (see paragraph 1, page 16).

Comment: The finding that ‘felt sad because not reunited with family members’ was no longer a significant
predictor of any mental ill measure (page 12) contradicts with the clinical experience and the literature mentioned (12,13, 32) does not support this finding either. I think more discussion is needed, what might be the background of this finding, while lack of (family) social support is so often found as a riskfactor for MH

Response: Good point! In the revised version of manuscript we have now included and discussed two non-mutually exclusive explanations for why this association is weak or perhaps even non-existent. We argue for example that it may be a consequence of how the question was formulated and it thus may be a poor proxy of social support or loss of close and important social ties (see paragraph 4, page 16).

Comment: Limitation

Response: Ok

Conclusion
The government can also be addressed when it comes to give more attention to adequately support I think . The PTE are in the past, but the PMS in the here and now and interventions can decrease them . Also health workers can be addressed, because they should seriously pay attention to the PMS and not only focus on treatment of trauma's rom the past. A resilient oriented approach is recommendable. If the comparative risks of PMS is found be higher that those of PTE your 'case 'is even stronger.

Response: yes and in fact, as the reviewer probably are aware of, several studies that have investigated the comparable risk between PTEs and PMS have indicated that PMS are more detrimental for refugees' mental health than PTEs. In this study no such comparisons were attempted, so we prefer to stick with a the perhaps somewhat unspecific conclusion that "Increased attention from multiple societal sectors to adequately support Syrian refugees' mental health needs, promote their recovery and reduce post-migration stress are needed.". This in order to avoid making too strong inferences that are not entirely grounded on the reported empirical finding from the present study.

Reviewer 2

Comment: The study examines, first, the prevalence and comorbidity of mental health problems (psychiatric disorders or symptoms of depression, anxiety, and posttraumatic stress, PTSD) in a representative sample of 1215 Syrian refugees settled in Sweden since 2011. Second, it analyses the role of traumatic war events and post-immigration stressors in associating with mental health problems (Mental ill health). The topic is very important and timely, as many refugee and asylum seekers attempt to heal their war-related psychic wounds alone, with the help of other people, and in the western health care systems. It is thus important to learn epidemiologically about the severity and contributing factors of mental health in the vulnerable group of refugees.

The study has some important strengths, including substantial sample size (although 70%-dropout),high internal consistence of main mental health variables, and professional statistics providing sensitivity analysis and large number of very detailed Tables. The manuscript serves criticism for the quality of scientific argumentation for the research questions (Introduction), including too old literature of trauma research and narrow reporting of earlier empirical findings among refugees in Europe. The manuscript would need more scientific conceptualization and structuring of Introduction (including current research questions) and the Method and Results sections. Hopefully the critical remarks below help the authors to rewrite their manuscript. A thorough rethinking and reworking would improve the manuscript.

Response 1: Thank you for pointing out the relevancy of the study and for your comments and suggestions. We agree that revisions has improved the manuscript extensively. We have tried to clarify the focus, objectives and the ambitions of the present work, which we believe were the primary sources of some of the misconceptions and ambiguities in the previous version. Other revisions also have been made in accordance with the comments by the reviewer. In our responses here, we have provided a more through discussion about our rationales and the primary epidemiological approach utilized in the present work, as we believe some of the criticism may stem from the format, treatment of empirical data and theory, and the mode of reporting that is inherent in this specific format.

Our responses and the description of the revisions made on the basis of the comments are outlined below:

Comment: Introduction. The introduction does not follow the principles of scientific writing. The authors should present more sufficiently and informatively the available research on mental health (mental ill health) among refugees in western or Nordic countries. They have chosen one review (Bogig et al.) that is adequate, but they have not conducted a thorough literature review on war trauma, mental health and on underlying psychosocial factors and processes. Learning about earlier literature would be necessary in order to be able to contribute to the field of refugees' mental health. Currently the reporting is highly technical, e.g., reporting so large differences as 2% and 80% in disorder prevalence is not informative. The introduction should be written in a more problem-focused and conceptualized manner. The reader is curious and wants to know e.g., why one refugee group would show 4% of PTSD, while in some another groups 86% suffer. If it is important to give this information (which I doubt), the authors should report that maybe the refugees with 86% disorders were a patient group,.seeking help in health care, or arrived just from concentration camp or something that makes sense. The current technical writing without problem-focused orientation is not informative. In addition to Bogig, the authors should review earlier research e.g. by C. Panter-Brick, M. Fazel, T. Beatancourt, E. Hauff, or D. Silove.

Response 2: We have now revised the introduction section substantially. The presentation have been reworked to become less "technical" and, in line with the suggested comment by the reviewer, we have supplied additional references that are more contemporary and from the European context. Furthermore, rationale for the objective of the study has been more clearly presented in the revised version. The following revisions and amendments have been made in the introduction section:

1. In the first paragraph of the introduction the following sentence have been added "A clear picture based on robust empirical data of the magnitude of mental ill health among Syrian refugees and to what extent they are, and have been, exposed to known risk factors are imperative to adequately address their mental health needs on a societal level." We hope that this explicit emphasis on the importance of the establishing the prevalence of mental ill health and its risk factors among Syrian refugees, which is the primary focus of this study, contributes to more clarity for the rationale and the objective of the study .

2. The second paragraph in the introduction has been revised to clarify the importance of acquiring up-to-date empirical data on the current prevalence of mental ill health among Syrians refugees rather than extrapolating from studies conducted among other refugee populations and within other contexts. We present the rationale that refugee populations often are very different with regard to several important aspects that influence mental health status. We further argue in this paragraph that many existing prevalence studies of mental ill health among refugees may be biased due to methodological shortcomings. The presentation of the disparate prevalence figures from Bogic et al's systematic review here merely serves to illustrate the heterogeneity of refugee populations with regard to mental ill health as well as variation among the methodologies used to examine these rates. We hope that these points contribute to more clarity in the presentation of the objective and rationale of the study.

Comment: The reference of Mollica et al. and Carlsson et al. are from 1990's and concerning Cambodian refugees in USA. Thus more contemporary literature should serve as an argument how the current research questions are contributing something new to the literature. The research task needs more clear conceptualized of the setting. The authors list issues like mental health, traumatic events and stressors as they would be the same phenomenon. They should make clearer what are e.g. war-related traumatic events and potentially traumatizing events (PTE), and clarify the relations between war trauma, post-migration stressors and psychiatric disorders.

Response 3: The sections about different refugee-related PTEs and types of Post migration stress have been expanded to provide a more specific rationale for the research questions. Alterations have also been made to explain how PTEs and PMS differ conceptually, that they may have different effect on mental ill health and that they to some degree also are empirically related. In addition changes have been made to clarify that the research questions, which are primarily correlational, in this study focus on how the specific types of PTEs and PMS included in this study are associated with mental ill health, rather than to provide an exhaustive account of to what extent the entire constructs of PTE and PMS predict mental ill health (see paragraph 3-4, page 4 & paragraph 1-2, page 5).

Comment: Research aims. Usually the introduction ends up with clearly stated aims/tasks/research questions/hypotheses of the study. Currently there is not a separate paragraph for the aims. Due to the poor conceptualization of the research setting (war trauma and refugee-immigration -related stressors), the research aims are highly explorative. In the Result and Discussion part authors write that some findings were "unexpected" or "expected", which means that they had had implicit hypotheses. The earlier research on the topic of refugee mental health and related factors is extensive, which legitimates the formation of hypotheses. The current writing is rather careless, e.g., authors state "second aim", but miss "first aim". The second research question should be reformulated so that it would be understandable as such, alone (e.g., not "four studied mental ill health measures"). The research task of analysing the prevalence of PTE and post migration stress (indicated by single items constructed to the present study) is not a scientifically important or new question, and would rather be placed in Descriptive statistics or Description of the sample.

Response 4: In the revised version, the aims are now presented in a separate paragraph. The "second" aim has also been reformulated to explicitly state that this aim is to investigate whether different types of refugee-related PTEs and post migration are associated with mental ill health among Syrian refugees, and that we in fact (as correctly is pointed out by the reviewer) expect to find positive associations.

We agree that the mere presence of PTEs and post migration stressors should not be viewed as new findings. However, we believe that establishing up-to-date prevalence rates of specific types of refugee-related PTEs and post-migration stressors bears justified relevancy as these factors constitute important determinants of mental health in current refugee populations. Yielding current and locally relevant estimate of these correlates and determinants, thus, not only corroborates earlier findings but also provide an overall picture of the present challenges and adversities that the refugee population are currently facing. In regard to public health policy purposes, we believe that providing empirical evidence on how common these type of adverse events or conditions currently are among particular refugee populations, contributes to more informed decision about the magnitude and targets for care, support and public health policy interventions in the host society.

The rationale for not placing the estimates of PTEs and PMS in the descriptive statistics section is that these statistics are population estimates of parameters rather than descriptive sample characteristics. This approach is based on epidemiological methods of estimation of unknown population parameters and as such constitutes an inferential procedure. Presentation of confidence intervals and weighting of each prevalence estimate effectively renders these analyses non-descriptive.

Comment: Methods. The section does not follow the “common” logic of research reports (also not according to the BMJ-open guidelines). It would be more reader-friendly to apply “traditional” reporting style. (Participants: Who participated, from where they were recruited, selection criteria etc., Study Procedure: How the study was conducted and how data were collected and analyzed. Measures: How the concepts were constructed to be assessed). Currently, the Method starts with Procedure (or even mailing of questionnaires), and the whole text (two pages) is difficult to follow, and the reader has difficulties to find the basics information. Drop-out or attrition analysis is missing…. The paragraph “Participant involvement” could maybe moved together with information on piloting the research tools and setting. (Currently defined as “Usability of the questionnaire was tested by cognitive interviews conducted in a rehabilitation center for war and torture trauma patients, with ten patients with Arabic as their mother tongue”).

Response 5: Thank you for pointing out the need for restructuring the methods section. The Methods section has now been revised so it includes the subheadings Participants, Procedure, Measures and Statistical analysis. Some sentences have, as a consequence, been omitted or reformulated. As suggested by the reviewer, we have also incorporated the paragraph “Participant involvement” into the “Procedure” section.

Comment: The Statistical analysis -section should be written so that it follows the research tasks or questions (after their reformulation).

Response 6: Given that Reviewer 1 found the structure of reporting of statistical analyses clear and adequate, we have opted to leave this section intact. We realize that the ordering of the statistical analyses can follow the research questions as suggested here, or be chronological, i.e. following the actual order of the analyses process. As this choice reflects preferences that sometime can be attributed to praxis within different disciplines, we have settled for the latter strategy. Our intention is thereby (as far as possible) to display the analytic strategy in the correct sequence so the reader easily can follow it. Although this may not be fully according to the expressed preferences by the reviewer, we hope that the reviewer still can find the present order informative.

Comment: Results. The results section is extensive with a large number of Tables (and Supplement tables). In this sense, the manuscript seems more a rapport than a scientific paper. The reformulation of research questions may help. Thus, prevalence of PTE and post-immigration stressors would not be research task but they belong to the descriptive statistics.

Response 7: The supplementary tables are included so the reader, if interested, can evaluate to what extent the results are robust and replicable, rather than the product of a more or less arbitrary analytical decisions (e.g., in regard to the cut-offs). These results are not included in the main manuscript. We hope that the responses to the previous comments serve to clarify the rationales for the inclusion of present research questions, more specifically, the estimation of prevalence of PTE and post-migration stress (please see Response 4).

Given the overall epidemiological approach of the present paper with a primarily focus on providing the estimates of unknown population parameters, we believe that the comprehensive statistical reporting and the observed similarity with empirical reports are somehow inevitable. We believe that despite the inherent difficulty in navigating among a large number of estimates, this information holds current relevancy.

Comment: It is difficult to understand why authors have chosen to analyze the association between trauma, stress and mental health by using single items of war-related traumatic events and post-migration stressors. More preferable would be to construct sum variables, and apply for instance step-wise regression models to learn what factors contribute to the refugees' mental health. The single associations seem more as a primary analysis.

Response 8: The rationale for assessing the associations between each individual exposure item (i.e. PTE and post-migration stress) and mental health outcomes is the intention to establish the relevancy of these specific adverse conditions and events for mental health of the targeted refugee population. Constructing a sum score to include in the analyses would implicitly require the assumption of an underlying unidimensional latent factor, which would need to be indicated by the number of types of exposures. Such a latent variable (i.e. number of types of PTEs or number of types of post-migration stressors) reflects a qualitatively different construct than that of individual exposures. Although "the number of types of exposure" is potentially an interesting variable to examine, the constructs needs more elaboration and requires a comprehensive list of exposures to be empirically informative. Moreover, the results of such analyses would not substitute the individual associations due to qualitative differences between these constructs. However and in order to not leave this interesting issue completely unanswered, we have now also included an additional supplementary mediation analysis (described at the end of the statistical section and in the sensitivity analysis section) in which we have analyzed the number of types of PTEs and PMS as two continuous variables to approximate whether the association between PTEs and mental ill health is partially mediated by PMS in terms of number of exposure types from the included pool in the study. Although such mediation analysis is crude due to sub-optimal elaboration of the assumed included latent variables, and a lack of exhaustiveness of and exclusiveness among the types of included exposure, this may serve as a point of departure for future analyses.

Our rationale for not carrying out stepwise regression analyses with number of types of exposures is based on the implied risk of biased estimates due to over-adjustment and the potential influence of both outcomes (mental health) and exposure (PTE) on the hypothetical explanatory variable included in the latter stages of the analyses (i.e. post-migration stressors). The resulting estimates, although likely biased, would also reflect a different research question such as following: "to what extent can the association between number of types of PTE and mental health be explained by the number of types of exposure to post-migration stressors". As such, analysis pertaining to this research question would fall beyond the aim and the scope of the present study. We hope that the provided information on the associations between specific exposures and mental health outcomes can be viewed as informative in its own right.

Comment: Discussion. The idea of discussion is e.g., to (1) summarize own main findings in a more abstract and theoretical manner (not only repeating the results), and combine them to earlier studies, phenomena and theories to illuminate the significant of the findings. (2) Discuss each main finding or combination of findings in relation to earlier studies. Building theoretical arguments to deepen the understanding of the phenomena is important (e.g., here the factors that explain the variation of ill health after potentially traumatized refugees, why does vulnerability occur). In the current text, there is not really discussion of the results, but the text repeats the findings. The idea of discussion is to integrate information, to elevate concrete findings on a more abstract (theoretical) levels. The discussion should thus be rewritten in the normative scientific ways.

Response 9: Thank you for your input on the structure and the idea of the discussion. The discussion section has now been revised and extended. We have provided a more detailed discussion about why some of the associations found in this study and the specific refugee population are not entirely in line with results obtained from other studies and populations. We also included a more thorough discussion of how the present study's prevalence rates obtained from this specific refugee population relates to those obtained from other refugee populations and in earlier studies. We have also included a brief discussion regarding the potential transferability of the estimated prevalence rates to other European countries. The presented structure of the discussion follows the STROBE- protocol and checklist (please see: von Elm E, Altman DG, Egger M, Pocock SJ, Gøtzsche PC, Vandenbroucke JP; STROBE Initiative. The Strengthening the Reporting of Observational Studies in Epidemiology (STROBE) statement: guidelines for reporting observational studies. Lancet. 2007 Oct 20;370(9596):1453-7. PMID: 1806473) as required by the editor.
We realize the value of further abstraction and theorizing in scientific work, specially pertaining to works in disciplines within humanity, social and behavior sciences. We are sure that the reviewer agrees that the extend of the application of theory and abstraction of empirical results within epidemiological studies, which is the primary pretext of the present study, is however not as extensive. This is reflected in the STROBE protocol and its associated explanatory publications (i.e. Vandenbroucke et al., 2007). According to STROBE, the discussion should summarize the key findings, relate to similar findings, discuss bias, validity, and generalizability, and only provide "a cautious overall interpretation". Furthermore a strict warning for over-interpretation is stated in the explanatory publications of the protocol. We believe that by taking into consideration this discipline-related contrast in regard to application of theory and our confirming with the STROBE protocol in presenting the discussion, our approach may appear more adequate.

Comment: The authors consider their research the first study "of this kind" and specify rightly "Syrian refugees settled in Sweden". Yet, that is not enough; the authors may discuss how and why Syrian war trauma may differ, e.g., from other war-survivors settled in more peaceful countries. The findings concur with earlier studies (women vulnerable, disorder prevalence, exposure to trauma associates with high risk for disorders). Maybe a more thorough familiarizing with existing research would have brought new contributions to the field.

Response 10: The rationale for the choice to refrain from discussing potential differences between Syrian war trauma compared to other war-survivors' extensively is that our empirical data does not provide support for such comparison. Consequently treating this matter would be rendered speculative and represent a departure from the STROBE guidelines. Neither was such a comparison, as interesting as it is, a part of the objective of the study. We do agree that framing our findings in empirical evidence of specificities of war trauma related to different refugee population would have been a much more substantive contribution to the field. Still, we hope that our choice of staying fairly close to our empirical findings could be viewed as informative.

Comment: Yet, the replications are also valuable, but the replication of earlier findings is here based here on single items of trauma and stress, which is open to criticism.

Response 10: This limitation has now been addressed in the discussion section (paragraph 5, page 17). We are aware that the use of single item assessment is contested, and although it is open to justified criticism there are also proponents of such usage (i.e. Hayduk & Littvay, 2012). Our choice was mainly due to the sensitivity inherent in the mode of data collection (survey) and the lack of necessary supportive settings that would be required for an elaboration of trauma-assessment given the potential risk of distressful reactions associated with more extensive and indepth assessment approaches.

VERSION 2 – REVIEW

REVIEWER	CornelisJ. Laban De Evenaar, North Netherlands Centre for Transcultural Psychiatry, GGZDrenthe, The Netherlands
REVIEW RETURNED	24-Oct-2017

GENERAL COMMENTS	The article has been well improved. The authors have taken the comments and suggestions into account. I repeat that is a very welcome and excellent study. My mayor comment in the first review was : how does post migration stress influence / moderate the relationship between PTE's and the mental health outcome measures . The authors analyze them PTE and PMS separately but it would be very interesting to also include them in one analysis . I recommend to do this, I was very happy when I read in the revised article the authors state at the subparagraph Statistical analysis at page 8 and 38 : " Finally, to examine the potential mediating role/function of post-migratory stressful experiences in the association between PTEs and mental ill health, mediation analyses were performed with number of included types of exposure for PTEs as exogenous, number of types of post-migratory stressful experiences as mediator, and mental ill health as endogenous outcomes. All analyses were conducted with SPSS v. 24.0 except the mediation analyses that were performed in Mplus version 8. " BUT : the results of these analyses are not shown at in the Results ! . Not in text and not in table. The findings of these analyses then should be discussed in the Discussion paragraph. I am looking forward to these additional analyses For the conclusion I repeat my notes : Conclusion The government can also be addressed when it comes to give more attention to adequately support I think . The PTE are in the past, but the PMS in the here and now and interventions can decrease them . Also health workers can be addressed, because they should seriously pay attention to the PMS and not only focus on treatment of trauma's rom the past. A resilient oriented approach is recommendable.

VERSION 2 – AUTHOR RESPONSE

Comment:
The article has been well improved. The authors have taken the comments and suggestions into account. I repeat that is a very welcome and excellent study.

My mayor comment in the first review was : how does post migration stress influence / moderate the relationship between PTE's and the mental health outcome measures . The authors analyze them PTE and PMS separately but it would be very interesting to also include them in one analysis . I recommend to do this.

I was very happy when I read in the revised article the authors state at the subparagraph Statistical analysis at page 8 and 38 :

" Finally, to examine the potential mediating role/function of post-migratory stressful experiences in the association between PTEs and mental ill health, mediation analyses were performed with number of included types of exposure for PTEs as exogenous, number of types of post-migratory stressful experiences as mediator, and mental ill health as endogenous outcomes.

All analyses were conducted with SPSS v. 24.0 except the mediation analyses that were performed in Mplus version 8. "

BUT : the results of these analyses are not shown at in the Results ! . Not in text and not in table.

The findings of these analyses then should be discussed in the Discussion paragraph.
I am looking forward to these additional analyses

Response: The result from these analyses which previously were described briefly in "sensitivity analyses" and among supplementary materials have now been incorporated in the main document. In order to make these changes fit better into the main result section and become more in focus, we have made some minor revisions in "statistical analyses" were these analyses are described.

A paragraph in the discussion section discussing the findings from these mediation analyses have also been incorporated

Comment:
For the conclusion I repeat my notes :

Conclusion
The government can also be addressed when it comes to give more attention to adequately support I think . The PTE are in the past, but the PMS in the here and now and interventions can decrease them . Also health workers can be addressed, because they should seriously pay attention to the PMS and not only focus on treatment of trauma's rom the past. A resilient oriented approach is recommendable

Response: We do agree and have in the revised version of the manuscript revised the conclusions in accordance with these valuable suggestions.

In addition we made some minor editing throughout the manuscript. Most importantly we have corrected some of the figures presented in table 4. These figures are the original figures on which the discussion and presentation of results are based.

VERSION 3 – REVIEW

REVIEWER	cornelis j. Laban De Evenaar, Centre for Transcultural Psychiatry North Netherands, GGZdrenth, The Netherlands
REVIEW RETURNED	27-Nov-2017

GENERAL COMMENTS	In my opinion the article is ready for publication now . Congrat